

θ cannot be thus seen, then it appears to me that all time spent in their search in the sun's vicinity, except during a total or very large partial eclipse, would be time lost.  
Rochester, N.Y., November 8      LEWIS SWIFT

#### Colour-Variation in Lizards.—Corsican Herpetology

IN a communication sent to you by my friend Mr. Wallace, under the title, "Remarkable Local Colour-variation in Lizards," published in *NATURE*, vol. xix. p. 4, mention is made of the well-known case of *Lacerta* (*Podarcis*) *muralis*, var. *faraglionensis*, only found on the Outer Faraglione of Capri, but there are many similar cases to my knowledge, and I add a note of them, for the fact, although unexplained, is one of great interest. During the last two years, while engaged in forming a complete series of the Italian vertebrate animals, I have visited and explored most of the Mediterranean islands included in the Italian sub-region, and I have invariably found that our common lizard (*Podarcis muralis*) constantly presents dark varieties on islets adjoining small islands: this is the case on the Scuola, near Pianosa, on the Scoglio di Mezzogiorno, off Palmarola (Ponza), on S. Stefano, off Ventotene, on the Toro, off Vacca (Sardinia), on Lisca nera, Lisca bianca, and Bottaro, off Panaria (Lipari), on Filifa, off Malta, and on Linosa, near Lampedusa. The extreme cases are those of the Faraglione off Capri and Filifa, where a nearly intense black is obtained; next comes Toro, and next Linosa; only the latter case might be explained by the "struggle for existence" theory, for the lava rocks of Linosa are black; but such is certainly not the case with the other islets, and, *pace* Dr. Eimer, the Faraglione is gray, while Filifa—on which I spent a pleasant day in October last—is painfully white in the glaring Maltese sun, so that its black lizards are most conspicuous. I may add that few creatures I know vary more in colour than *Podarcis muralis*, even in the same locality; two most distinct varieties occur promiscuously on the small flat islet Formica di Grosseto.

Going over my Mediterranean herpetological notes reminds me of an interesting discovery I made last summer in Corsica, an island of great interest, which, strange to say, is rarely trodden by naturalists. Most of your zoological readers will be aware that, in 1839, Prof. Savi, of Pisa, described two new species of Italian Urodela, both from Corsica, viz., *Salamandra corsica* and *Megapterna montana*. The former has been quite neglected by modern herpetologists, or else placed among the synonyma of *S. maculosa*, simply because no one had Corsican specimens to compare. Now it is evidently nearly allied to the Continental form, but quite distinct, as the specimens I collected testify, all of them presenting the distinctive characters pointed out by Savi forty years ago. A nearly similar lot befell *Megapterna montana*, which Savi described nearly contemporaneously with Gené's description of *Euproctus Rusconii*, from Sardinia. Buonaparte, in his "Fauna Italica," united the two under the name of *Euproctus platycephalus*, given by Gravenhorst in 1829 to a newt, *sine patria*, preserved in the Breslau Museum; and most naturalists have followed Buonaparte, especially later writers on the subject, as Strauch, De Betta, and Schreiber, whilst others, acting more wisely, stuck to Gené's name. I believe that since Savi's day no one has studied the Corsican form, whose essential characters pointed out by the Pisan naturalist, who had only two specimens to work on, were overlooked even by his contemporary, the Prince of Canino; this explains all. Last year I rambled and collected all over Corsica, and found Savi's newt quite common in all the mountainous districts; I secured about 150 specimens of both sexes and all ages, even larvae, and on my return to Florence was much surprised to find them quite distinct from the Sardinian *Euproctus* I possess; this made me refer to the original descriptions, and thus I found that Savi and Gené had described two very distinct species, and described them well. The two Italian species of *Euproctus* may be thus defined:—

*E. Rusconii*, Gené: Parotids wanting. Skin smooth, with small whitish tubercles scattered, especially about the sides of the head and neck. Female with a small conical pointed fibular tubercle, very like a rudimentary finger. Hind fingers slender and cylindrical. Irregular dark blotches on the throat. Size somewhat larger than the succeeding species. Hab. Sardinia.

*E. montanus*, Savi: Parotids small but distinct. Skin rough and granular. Female with a large, obtuse, compressed fibular tubercle, more like a ridge or crest, than anything else. Hind fingers stout, broad, and flattened. Throat uniform, rusty,

without blotches; often a red or yellow dorsal stripe. Hab. Corsica.

As to *Euproctus platycephalus*, Gravenh., only a careful examination of the type-specimens, if yet existing in the Breslau Museum, can settle to which form it ought to be referred, but if their locality is unknown, I believe it better to suppress the name. *Euproctus platycephalus* is said to be found in Spain, but as I have no Spanish specimens, I cannot give any opinion on that form. In conclusion, I may add that Buonaparte was perfectly right in separating from the former the North African species *T. Poiréti*, which is very distinct from our Italian *Euproctus*, in the shape of the head and body, and in the complete absence of any fibular tubercle in the female; it ought to be called *Glossoliga Poiréti*.

HENRY HILLYER GIGLIOLI

Florence, November 16

#### Commercial Crises and Sun-Spots

REFERRING to Prof. Stanley Jevons's article upon "Commercial Crises and Sun-spots" in *NATURE*, vol. xix. p. 33, I beg to draw your attention to the inclosed circular which I issued to my subscribers in April last.

The figures relating to the "Failures in England and Wales," were compiled by my clerks, under my own direction; those relating to the failures in the United States and Canada were supplied by Messrs. R. G. Dun and Co., of New York and London, and it may be observed how nearly they agree (*i.e.*, the failures in England and Wales, and those in the United States and Canada) in their fluctuations, and that there is an agreement between both sets of figures and the sun-spot period.

I have not been able to obtain similar figures for continental states, but I have observed that the complaints of depression in trade there agree, in substance and in time, with those in this country and North America. I have also noticed similar complaints from the southern hemisphere, especially New Zealand.

I refer to Dr. Hunter's suggestion of an Indian famine period in my circular, but I do not find that the famine period in India agrees, in point of time, with the depressions in the temperate zones; it is very probable that the excess of sunshine which produces drought and famine in India has an opposite effect on the prosperity of England and all other countries lying between the same isothermal lines, and that the more moderate degree of sunshine which may suit the Indian cultivator is insufficient to properly ripen English wheat and other produce (oats excepted).

Since April last I have taken several opportunities of ascertaining from agriculturists the effect of the variations in the sun-spots upon their yield of wheat, &c., and I find an agreement between them that during these years of minimum sun-spots the yield has proved bad when thrashed out, in consequence of the kernels being much smaller than in other years. I do not know whether the test has ever been tried or not; if not, I would suggest that some scientific observer should weigh an ounce, or a few ounces, of the kernels of each kind of grain grown in England every year, and count the number of them. I think it would be found that in years of maximum sun-spots wheat and barley kernels weigh their heaviest and oats their lightest, and that these proportions would be reversed in the years of minimum sun-spots. The difference in each kernel or in an ounce of them may, taken alone, appear trifling; but if it is an indication of the difference in the yield of the harvest throughout the whole kingdom, it may be a fact of the greatest importance as showing the cause of the cyclical variations in the prosperity of the country, and it may be of great value to land-owners and agriculturists generally as a guide in the rotation of crops and in allowing fields to lie fallow.

It is in this direction that I look for the causes of commercial depression. The whole of our "home" trade is dependent upon internal prosperity, and likewise a large proportion of our "foreign" trade. Other causes may have some effect upon either or both, such as peace or war, trade-unionism, bank-management, and the like; but the influence of the sun is too far-reaching and too powerful to be checked thereby. Man, by studying the working of its influence and power upon his daily life, may learn how to guard against much of the distress which periodically recurs.

JOHN KEMP

Aspley Guise, November 16

"London, April, 1878

"Failures in England and Wales

"We append a Summary of the failures in England and

Wales, which it has been our duty to publish in *Kemp's Mercantile Gazette* during the past eleven years:—

Year.	1st quarter.	2nd quarter.	3rd quarter.	4th quarter.	Totals.
1867	3,981	4,081	3,555	4,233	15,850
1868	4,091	4,131	4,139	3,501	15,862
1869	3,819	3,997	3,495	5,207	16,518
1870	2,804	1,589	1,773	1,985	8,151
1871	2,142	2,191	1,837	1,994	8,164
1872	2,192	1,980	1,795	2,145	8,112
1873	2,354	2,299	2,054	2,357	9,064
1874	2,193	2,428	2,339	2,290	9,250
1875	2,331	2,277	2,133	2,453	9,194
1876	2,744	2,573	2,670	2,861	10,848
1877	2,829	2,856	2,610	2,952	11,247
Total for 11 years					122,260

"The question occurs: Does the number of failures in a year depend upon natural causes? that is to say, Would the number rise and fall periodically according to the state of trade (or national prosperity) if the Bankruptcy Law remained constant? Whenever failures have become frequent, complaints have been made against the Law, and not without reason, but many who complain ignore the existence of any other cause. We compared the foregoing figures with the scientific tables recently published in *NATURE*, from the pen of Prof. Balfour Stewart,<sup>1</sup> and, being struck with the coincidence in their fluctuations, we further compared them with the statistics published by Messrs. Dun and Co., of New York, of the failures in the United States during the past eight years,<sup>2</sup> which period, being that of the existence of our present Bankruptcy Law, affords us a fair opportunity for making a comparison. Messrs. Dun and Co. report the following as the total failures in the United States during this period:—

1870, Number of Failures,	3,551
1871, " "	2,915
1872, " "	4,069
1873, " "	5,183
1874, " "	5,830
1875, " "	7,740
1876, " "	9,092
1877, " "	8,822

"Evidently the same causes which were at work in England to depress trade and overwhelm the struggling and improvident classes, were equally effective in other countries—similar complaints of depression come to us from every part of the globe.

"The discussion which has arisen out of Dr. Hunter's suggestion of a 'famine period' in India, has brought to the public some knowledge of the existence of natural periods or cycles, of an average duration of 11·9 years each. The suggestion that England is affected with the same regularity is but reasonable, and although fortunately for us as a nation the effects do not produce famine, it appears evident that some degree of suffering is caused, and that the number of failures is thereby materially increased—the commercial panics which have occurred with about the same regularity furnish further evidence that this is the case.

"If we make due allowance for the excessive number in the last quarter of 1869, caused by the change in the Law, we find that the maximum number of failures in the last cycle occurred in the year 1868, which was the year succeeding the natural minimum; hence we may conclude that about a year is required for the full effect of the natural depression to be reproduced in commerce. The twelve months from October 1, 1867, to September 30, 1868, appear to have been more serious to commercial men than either of the complete years, according to the number of failures:—

"In the 4th Quarter of 1867 there were 4,233 failures.				
" 1st "	1868	"	4,091	"
" 2nd "	1868	"	4,131	"
" 3rd "	1868	"	4,139	"
Total ... .. 16,594				

"These data indicate that we have not yet reached the worst of the present period—assuming that it runs an average length,

<sup>1</sup> Vide *NATURE*, vol. xvi. pp. 9, 26, 45.

<sup>2</sup> Messrs. Dun and Co.'s Annual Circular, January, 1878.

we have to endure an increasing number of failures which will not reach its maximum until the fourth quarter of 1879."

JOHN KEMP AND CO.

Since this was written I have had counted the number of failures gazetted since January 1, 1878, and I find that they are 2,042 in excess of the corresponding period (January 1 to November 19) in the preceding year.

J. K.

### Strange Properties of Matter

THE following are two experiments which will, perhaps interest some of your readers:—

#### Experiment No. 1.—The "Welding" of Metals at Low Temperatures

Some time ago, in order to estimate the amount of hydrocyanic acid in a solution, I precipitated it with silver nitrate. After having filtered and washed the precipitate, I reduced it to the metallic state by heating to the required temperature. Just as I was about to allow it to cool, I noticed a small piece of dirt among the reduced silver. In order to separate them, I took a thin platinum wire, and pushed the silver to one side, but on attempting to take the wire away the silver remained in contact with it. As I thought this curious, I tried the following experiment. I took a piece of silver foil about one centimetre square, placed it in an inverted porcelain crucible lid, and heated it to about 500° C.; then I brought into contact with it the extremity of a thin platinum wire, and to my astonishment the wire raised the silver from the lid, and it remained in contact when cold, as the silver was so very much below its melting-point; the above fact caused me some surprise, and I could not satisfactorily account for it.

I wrote to Sir W. Thomson, F.R.S., giving him a description of the above experiment, and in return I received a reply asking me to come and show him the experiment at his laboratory. I accordingly went up to the Glasgow University, and repeated it before him. He was very much interested, and advised me to write to *NATURE*, giving a description of the experiments. Sir W. Thomson gave the following explanation—That it was a remarkable case of "cohesion," the two metals, in fact, "welding," although the temperature was far below the melting-point of silver. The above experiment can be performed successfully at lower temperatures than 500° C., if smaller pieces of foil are taken. Other metals, for instance, copper and aluminium, cohere to silver in the same manner as platinum, but less strikingly.

#### Experiment No. 2.—A Curious Resonator

Some months ago I made the following experiment:—I took a small tuning-fork and struck it on the table. After the note had died away, so that it was no longer audible, I held the fork in the tip of the flame of a Bunsen burner, when the note was given out, so that it could be heard at some distance. I showed Sir W. Thomson this experiment, who gave the following explanation—That owing to the difference in density of the gases in the flame, the flame acted as a resonator, and so the note was emitted.

It seems to me that experiment No. 1 could be made the subject of an interesting research, but as I am wholly engaged in commercial pursuits, I am unable to take it up.

CHARLES A. FAWSITT

Glasgow, November 12

### Galvanometer for Strong Currents

I MUST confess that I was surprised by Mr. R. E. Baynes' communication, in *NATURE*, vol. xix. p. 33, that the galvanometer I have proposed in *NATURE*, vol. xviii. p. 707, has already been described. Before writing my article I have searched a good many books and journals relating to the subject without finding an allusion to any such instrument. Since Mr. Baynes drew my attention to "The Elements of Physical Manipulation," by Prof. Pickering, of the Massachusetts Institute of Technology, U.S., I have procured this book and find that it certainly does contain the theory of a galvanometer like mine, with the coil moving round a horizontal axis. As far as I know, such an instrument has, however, not been practically employed either in this country or on the Continent before I introduced it, though its want must have been much felt for some time past. This seems to show that Prof. Pickering's description of the instrument has not been brought to the general